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REMARKS

ON

UEKETT'S HISTOLOGY, ON KÖLLIKER'S HUMAN HISTOLOGY,

AND ON

THE PHYSIOLOGICAL IMPORTANCE OF THE NUCLEUS OF THE CELL.

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HE lectures delivered by Professor Quekett at the Royal College Surgeons of England, on the Elementary Tissues of Plants d Animals,[1] give a most comprehensive view of a widely extended bject, without any neglect of minute details. And, far from being repetition of what others had described, they are everywhere ırked with evidence of original research—research, too, in a col- tion of preparations that has no rival, and evidently with a keen croscopic eye.

The chapter on "cells," in the translation of Kölliker's "Manual Human Histology," vol. i.,[2] furnishes evidence of progress, deeply eresting to one who sees in all the figures, and in many of the thor's remarks, confirmations of facts which he himself, the author the present communication, had recorded fourteen years before.

When the microscopic inquirer has become acquainted with portant facts, and left no means untried for testing the accuracy his observations, it is his duty to record the same, however much osed to existing views; for if he has faithfully represented nature his drawings, he may "bide his time," resting assured that the will come when others will see what he has seen. Of this

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my own experience has given me many proofs. The volumes by Quekett and by Kölliker furnish many more. Such at least, I think, will be found the case by those who may feel disposed to compare the drawings of these excellent observers with my own.

In 1840, when my views on the physiological importance of the nucleus of the cell were announced,[1] I had the misfortune to stand quite alone; for no one then had questioned the views of Schleiden and Schwann, that the nucleus, after forming the membrane of the cell, "either remains inert," or, "as a useless member, is absorbed." A separation of the nucleus into two or three parts, where previously observed—viz., in the globule of pus and mucus—had been attributed by Güterbock, Henle, and others, to a chemical reagent used in the examination—acetic acid.[2] But I showed that neither acetic acid nor any other foreign substance is required to produce the separation in question; this separation being natural, apparently common to nuclei in general, and forming part of the process by which cells are reproduced. Farther, I showed that young cells originate through division of the nucleus of the parent cell, instead of arising as a sort of "product of crystallization in the fluid cytoblastema" of the parent cell, as since supposed by Reichert.[3]

The following are some of the principal facts which I recorded—the first three in the Phil. Trans. for 1840, and the others in the Phil. Trans. for 1842:—

1. The nucleus neither "remains inert," nor "is absorbed as useless," after the formation of the membrane of the cell.
2. The cell is propagated by division of its nucleus.
3. This division of the nucleus is initiated by division of the nucleolus.
4. Fibre is *originally* flat, and marked with striæ, and it has *originally* a crenate edge.
5. Fibre shows a constant tendency to lose its striation, and become rectilinear.
6. In some cells the nucleus is seen giving off fibre.
7. Sometimes the entire nucleus becomes a *ring* of fibre.
8. Sometimes the entire nucleus passes into a *coil* of fibre.
9. Sometimes fibre given off by the nucleus of a cell is met with, simply deposited upon the inner surface of the cell-wall.
10. Fibre given off by the nucleus of a cell sometimes thickens the cell-wall.
11. Fibre is sometimes seen entering into the formation of membrane.

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[1] "*Researches in Embryology, Third Series; a Contribution to the Physiology of Cells.*"—Phil. Trans. 1840.

[2] Henle, Hufeland's Journal, Mai 1838, p. 62, Pl. I, figs. 9-12.

[3] Das Entwickelungsleben im Wirbelthier-reich, 1842, p. 2.

12. The axile fibre of nerves has a structure sometimes giving it transverse striation and a crenate edge.

13. The "white substance" of nerves has a structure giving it originally transverse striation.

14. A mother-cell is sometimes seen divided into two compartments by two daughter-cells, whose walls are composed of fibre, and where in contact with one another form a thick septum.

Is there to be found among the figures given by Professors Quekett or Kölliker anything like a confirmation of these facts?

1. *The nucleus neither "remains inert," nor "is absorbed as useless" after the formation of the membrane of the cell.*—Phil. Trans. 1840.

Compare with the following by Kölliker in 1853:—"Both kinds of cell-formation agree in this, that the cell-nuclei play a very important part, and appear to be the proper centres of development (Bildungspunkte) of the young cells."—*Lc.* p. 19.

2. *The cell is propagated by division of its nucleus.*—Phil. Trans. 1840.

Compare with the following by Kölliker in 1853:—"The first thing to be observed in this case" (the development of cells within other cells), "in the parent cell, is a metamorphosis of its nucleus, which grows, acquires two nucleoli, becomes elongated, and divides into two."—*Lc.* p. 22.

3. *This division of the nucleus is initiated by division of the nucleolus.*—Phil. Trans. 1840.

Compare with the following by Kölliker in 1853:—"Cell-nuclei are produced endogenously in nuclei, or by their division under the influence of a nucleolus, which also divides."—*Lc.* p. 33.

4. *Fibre is originally flat and marked with striæ, and it has originally a crenate edge.*—Phil. Trans. 1842. More than two hundred drawings from nature.

Compare with Quekett's fig. 23, of a moniliform fungus occupying many of the cells, and threading its way between others, in diseased potato. Compare also with his fig. 127 *b.* from an enchondroma. In the latter his "concentric laminated deposit" appears to consist of my flat striated fibre. See, besides, Quekett's fig. 103 *b.* of striated fibres from the ligamentum nuchæ of the giraffe. He mentions having seen striated fibres also in the rhinoceros and the sheep, and even in arteries. (Among my figures will be found one of an artery from the pia mater with two strata of striated fibres, the inner stratum longitudinal and the outer stratum being a spiral fibre transversely crossing these. Phil. Trans. 1842. Plate VI., fig. 16.)

Compare with the following remark of Kölliker:—"The margins of the elastic fibres are in general quite rectilinear, but in some rare cases appear to be notched." (*Lc.* p. 61.) Kölliker's notches show what the original structure had been, just as the toothed fibres of the crystalline lens according to my observations are the remains of the structure of which they were originally composed. Phil. Trans. 1842, p. 104. Though in the lens we have a permanent, but probably in the elastic "notched" fibres only a transition state.

5. *Fibre shows a constant tendency to lose its striation, and become rectilinear.*—Phil. Trans. 1842, p. 92.

Compare with Quekett's fig. 18, of "elongated cells" of boletus igniarius, and with his description of fibres of the shell-membrane of the bird's egg. (*Lc.* p. 123.) (This shell-membrane was mentioned by me among the parts in which I had found fibres presenting the original striation, (Phil. Trans. 1842, p. 91), which seems to have altogether disappeared in those seen by Professor Quekett). Farther, compare in Quekett's fig. 105 of connective tissue, the smooth fibres *e*, and the striated fibres *a*, with my fact, No. 5, now mentioned; that fibre shows a constant tendency to lose its striation. For *a* in Quekett's fig. 105 exhibits a very early, and *e* a later stage.

Compare with Kölliker's fig. 28, of fibres of connective tissue exhibiting "the complete absence of striation;" a state, as I have said, which fibre shows a constant tendency to assume. Kölliker, however, had no idea of such a change, for he mentions this complete absence of striation as among the peculiarities distinguishing the fibrils of connective tissue from "their nearest allies, the finest elastic fibres and muscular fibrils." (*Lc.* p. 70.) Future research will, I have no doubt, convince this observer that originally there is no peculiarity in this respect—the fibrils of all tissues presenting originally a like striation.

6. *In some cells the nucleus is seen giving off fibre.*—Phil. Trans. Plate X., figs. 133 to 136 from cartilage of the ear, and in Plate V., many figures of cells in coagulating blood.

Compare with Quekett's fig. 6, from *opuntia vulgaris*, and with his figs. 124 and 127 *b*, from an enchondroma.

7. *Sometimes the entire nucleus becomes a ring of fibre.*—Phil. Trans. 1842. Plates V., VI., figs. 4 and 17.

Compare with Quekett's fig. 6, of cells with thickened fibres from *opuntia vulgaris*, and with his fig. 84 A. The quoit or ring-shaped bodies there seen, "affording evidence of concentric deposit," appear to be altered nuclei of cells.

8. *Sometimes the entire nucleus passes into a coil of fibre.*—Phil. Trans. 1842. Plates V., VI., XI., about fifty drawings of nuclei which have become coils of fibre.

Compare with Quekett's fig. 6, of cells from *opuntia vulgaris*, and with his fig. 84 A. The spirals there seen I apprehend to have arisen from the union of many coils, each coil an altered nucleus.

9. *Sometimes fibre given off by the nucleus of a cell is met with, simply deposited upon the inner surface of the cell wall.*—Phil. Trans. 1842. Plate V., figs. 8 to 12.

Compare with Quekett's fig. 84 A, of a short spiral vessel. And what a display of fibre, given off, as I believe, by a nucleus, is seen in his drawings of fibro-cellular tissue, figs. 61 and 62. It would seem that here, as the nucleus gives off fibre, what is left of the nucleus continues parietal to the last, and that it entirely exhausts itself in the formation of one long spiral fibre, the end of which (see the figure) denotes the position of what was the nucleolus.

10. *Fibre given off by the nucleus of the cell sometimes thickens the cell-wall.*—Plate X., figs. 133, 134, 136, from cartilage of the ear.—Phil. Trans. 1842.

Compare with Quekett's figs. 124 and 127 *b*, from an enchondroma.

Compare with many figures given by Kölliker. For instance, with that of the cells of cartilage from the epiglottis, fig. 21; with that of cells of the ligamentous discs of the bodies of the vertebræ, fig. 122, described as representing "*concentrically* thickened walls and *shrunken nuclei;*" with that of cells of costal cartilage, fig. 124 *c;* and with that of cells in rachitic bone, fig. 132.

When fibre is simply deposited upon the inner surface of the cell-wall, it represents, as it were, a commencement of the thickening.

Compare this remark, and my figures of 1842, with the following by Schacht in 1853:—"The increase in thickness of the cell appears to arise from the deposit of solid substance in the interior, upon the original cellulose of the cell-wall. This deposit of new matter frequently appears to take place in the form of a spiral; in very young wood-cells, for instance, in the youngest cells of a fresh twig of pinus abies, a most delicate spiral band may be observed in spring and in summer; in the older cells it can hardly be perceived. The band which is seen in spiral cellular tissue, the markings in the thickening layers of the liber-cells of vinca minor, the arrangement of those spots the thickness of which is less than that of the rest of the cell, and which occur in the thickening substance of the wood-cells of caryota urens and hernardia sonora, as well as the disposition of the slit-shaped pores of many wood-cells (those of cycas, for example), all afford arguments in favour of the thickening substance being deposited in the form of a spiral."[1]

Farther, compare these remarks by Schacht in 1853, with the following by myself, published in the Phil. Trans. for 1842, concerning some appearances which I had seen in plants :—"These spirals appeared to interlace with one another; and, by their close contact (fig. 90), to produce the appearance of transverse and elliptical 'pores' or 'dots.' The apparent 'dots' or 'pores' I believe were no other than spaces between the winds of spirals, contained within a tube. Now, spirals *interlacing* in the above way (figs. 87, 88, 90) must, by a longitudinal succession of their winds, produce *septa* (fig. 90 *a*) in the containing tube. I cannot help believing that these observations will assist to solve the still undecided question as to the structure of the 'dotted duct.'"—Phil. Trans. 1842. P. 108, § 83; plate VIII., figs. 87 to 90.

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[1] "The Microscope in its special application to Vegetable Anatomy and Physiology. By Dr Hermann Schacht. Translated by F. Currey, Esq., M.A." London: Highley. 1853. Pp. 33, 34.

The "manifest lamination in the thickening substance," mentioned by Schacht as seen in almost all cells of great thickness—for example, in many wood-cells—I apprehend to arise, either from the flat form of fibre, or from the splitting up of this into fibrils, which then adhere or coalesce so as to produce the said lamination.

11. *Fibre is sometimes seen entering into the formation of membrane.* —Phil. Trans. 1842. Plate VIII., fig. 69, where a spiral fibre is seen to have undergone division in order to form the sarcolemma. See also Phil. Trans. 1842. Pp. 92, 62.

Compare with Quekett's fig. 87, of hairs from the fruit of *cycas revoluta* unrolling spirally. And see his remark, illustrated by that figure :—"While speaking of fibres unrolling spirally, I take the opportunity of showing a fact I have lately made out, that even membrane itself may occasionally be met with, that will tear and unrol spirally."—*Lc.* p. 100.

In 1842, I stated that spirals constantly exhibited a tendency to pass into membrane, gave drawings of cells, the walls of which were made up of spiral fibres, and, as just mentioned, showed spiral fibres to divide and coalesce, and thus produce the sarcolemma.

Compare also my fact, No. 11, with the following remarks by Kölliker on elastic tissue :—"A modification of this fibrous elastic network is formed by the elastic membranes, in which the fibres are so closely interwoven that a connected membrane arises, which in the most extreme cases no longer exhibits any indication of its previous nature, and appears as a perfectly homogeneous membrane with smaller gaps (the fenestrated membrane of Henle)."—*Lc.* p. 61.

"The periosteum of the primarily cartilaginous bones" is described by Kölliker as "consisting, as early as at the fifth month, of common connective tissue and fine elastic filaments."—*Lc.* p. 361.

On "Elementary Processes in the layers formed from the Periosteum," Kölliker remarks, that "The matrix of the bone arises from its fibrous tissue by the simple uniform deposition of calcareous salts."—*Lc.* p. 362.

Cartilage affords a striking instance of formation out of fibre. The softer central substance of the intervertebral ligaments may serve as an example. On this subject, it is said by Kölliker: "The more central portions of this fibrous substance gradually pass into a thin, hard, yellowish lamella of true cartilage."—*Lc.* p. 315.

In 1852, Agardh showed the cell-walls in *conferva melagonium, griffithsia equisetifolia,* and *polysiphonia complanata,* to consist of fibre.[1]

12. *The axile fibre of nerves has a structure, sometimes giving it transverse striation and a crenate edge.*—Phil. Trans. 1842. Plate VIII.; figs. 80, 81, 85, 86.

Compare with Kölliker's remark, that this axile fibre,[2] in some instances, is "*striated*" (*lc.* p. 392), and sometimes presents a "jagged border" (*lc.* p. 396). If my drawings be referred to, the *structure* will be seen that produces striæ and a jagged border—my crenate edge.

13. *The "white substance" of nerves has a structure giving it originally transverse striation.*—Phil. Trans. 1842. Plates VIII., IX.; figs. 72, 77, 80, 81, 82, 99, 102.

Compare with the following from Kölliker: "The formation of the fibres in the cord and brain is extremely difficult of investigation, and is best studied with the aid of chromic acid. In the human embryo, I find, as early as the end of the second month, the commencement of the formation of the tubules in question, the white substance being distinctly finely *striated*."—*Lc.* p. 491.

The "distinctly finely striated" appearance here described by Kölliker, as seen in the white substance, I think will be found to have been explained by myself in the Phil. Trans. for 1842, in these words: "The filaments, which I believe to constitute the white substance of nervous fibre, are often seen to be, not longitudinal, but curiously interlaced (fig. 102γ), as though each filament ran in a spiral direction" (p. 97). In the figure now mentioned, it will be seen to be not the structure but the *direction* of the filaments that produces striæ.

Other figures besides the one referred to in the last paragraph were given by myself in the Phil. Trans. for 1842. And as they serve to explain the striæ seen in the white substance by Kölliker, as well as the occurrence of *varicosities*, I will trespass upon the reader's patience by quoting from my own writings a few lines more. "The frequent interlacing (fig. 102γ), and apparently spiral direction of the filaments in nerves, now referred to, seem the more deserving of attention, from my having found spirally directed filaments so very general in the retina, brain, and spinal chord (figs. 17 to 22, 72, 77, 80 to 82, 85, 99). Farther, I have noticed spirally directed filaments, on being broken, to recoil, (figs. 80, 81). Such a change in the white substance taking place within a tube, *might produce varicosities*, and those minute isolated masses hitherto called granular, by which it has hitherto been usual to distinguish nerve."—Phil. Trans. 1842. P. 97.

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[1] "*De cellula vegetabili fibrillis tenuissimis contexta.*" Lundæ, 1852.

[2] "*Central* or *axis-fibre* of the nerve-tube," "*primitive band*" of Remak, "cylinder axis" of Purkinje.

My figures now referred to seem also to explain the granular appearance of the "sheath of nerve;" for instance, in Kölliker's fig. 158 *b*, which granular appearance I conceive to be referrible to more or less of a recoil of spirals. (See the preceding paragraph).

14. *A mother-cell is sometimes seen divided into two compartments by two daughter-cells, whose walls are composed of fibre, and where in contact with one another form a thick septum.*—Phil. Trans. 1842. Plate XI.; fig. 150.

Compare with Kölliker's figs. 123, 124, 132.

I think that this comparison of some of the drawings and remarks of Professors Quekett and Kölliker with my own, shows a confirmation of the facts just mentioned, as having been published by myself in 1840 and 1842.

To Quekett's statement, then, that the mode of formation of the spiral fibre in spiral vessels of plants is "by a secondary deposit in a spiral direction on the internal surface of the cell-wall" (*lc.* p. 95), I have to add that this deposit is effected *by the giving off of fibre in the form of a ring or coil by the nucleus.* And as "all the forms of vascular tissue are referrible to the same origin, this simple cell with its secondary interrupted deposit" (*lc.* p. 95), it follows that all these forms of vascular tissue are referrible to altered *nuclei.*

The term "cell" is often used when nothing is seen but a nucleus, the membrane of the cell having more or less completely disappeared.

This was noticed in cartilage of the cuttle-fish by Quekett, for he says, "Around some of these cells there is a faint trace of cell-wall, and it becomes a question whether many of them should not be considered as altered nuclei." (*Lc.* p. 139). And in fig. 127, he demonstrates the truth of Todd and Bowman's view, that the lacunæ of bone are developed from the nuclei of cartilage cells.

What Kölliker terms bone cavities or "cells" and canaliculi—*lacunæ et canaliculi ossium* (*lc.* p. 300, figs. 117, 119)—are therefore not cells but *nuclei*, and processes sent out by these.

Now, though in vegetables the cell-wall remains, yet just as the lacunæ of bone are developed from nuclei, so it appears to me is the *sclerogen* of plants (Quekett, *lc.* figs. 44 to 48), notwithstanding that anastomosis of the radiating tubes or canaliculi is here prevented by persistence of the cell-wall.

Farther, *Raphides* in their several crystal forms, whether acicular, conglomerate, stellate, octohedral, prismatic, etc., (Quekett, *lc.* figs. 27 to 33), *Phytozoa* (Quekett, *lc.* figs 58, 59), the crystals of *Margarine* and *Spermaceti* (Quekett, *lc.* figs. 141, 142), and a host of other things are, I think, formed by the nucleus of the cell. Even *Silica* itself, which Quekett is no doubt right in considering a secretion (*lc.* p. 56), analogy seems to warrant us in believing to be secreted by the nucleus alone.

On crystals of oxalate of lime, familiar to all who are in the habit of examining the deposits from urine, Quekett remarks that in this fluid he has found them "contained in the interior of cells, so that, both in the animal and vegetable kingdoms, we have crystals of oxalate of lime as a product of cell secretion." (*Lc.* p. 44). I would only add, that such crystals, as well as all others in both kingdoms, are no doubt formed by the *nucleus* or represent the *nucleus* in altered forms.

It is remarked by Quekett regarding the size of "the bone cells" or lacunæ: "I have ascertained that they bear a direct relative proportion to that of the blood-corpuscles." (*Lc.* p. 174). This is a confirmation of a fact which I had published long before as the result of wide research: that the larger the blood-corpuscles of the animal, the larger are the elements of the tissues.

Nowhere have my views on the physiological importance of the nucleus of the cell received more ample confirmation than in the article "Semen" of Todd's Cyclopædia, by R. Wagner and R. Leuckart, who constantly refer to Kölliker also as advocating an opinion coinciding with their own. Their figures present a vast number of instances, including both ends of the animal kingdom, in which a single spermatozoon is represented coiled like a young animal, having evidently arisen out of the nucleus of its cell. Farther, it appears to me from some of the same figures that the essential part, *i.e.*, the head-like end or body of the spermatozoon, around which the tail is coiled in its cell, arises out of the *nucleolus*. It is true they arrive at the conclusion that the mode of development is three-fold, viz., "1st, The cell membrane and nucleus of the formative vesicles convert themselves immediately into the spermatozoon. 2d, The nucleus of the formative vesicles alone metamorphoses itself into the spermatozoon. 3d, A new formation, which takes place in the interior of the nucleus (or immediately in the cell cavity), performs the functions of a spermatozoon." But if they saw some spermatozoa to arise, "simply and immediately from a metamorphosis of nuclei" (*lc.* p. 501), these observers may be quite sure that the part performed by the "cell-membrane" (1.) is very subordinate; and as to the "cell-cavity" (3), their own remarks show it to be by no means certain that the developing cells or vesicles are not "in all cases nuclei."—*Lc.* p. 499.

That these "developing cells" or "vesicles" are really nuclei in mammalia, was shown by myself from observations on the rabbit as long since as in 1841. Those observations were incidental, but may be referred to here. It would seem that in this animal there exists first a large cell $\frac{1}{100}'''$—$\frac{1}{80}'''$,—that the nucleus of this large cell divides into many parts which are smaller nuclei,—that each of these smaller nuclei has its own investment, and that they are the "developing cells" of authors which give origin to the spermatozoa.[1]

The products of the testis have, it may be presumed, their equivalents in the ovary; and it happens that the very plate containing my drawings now referred to, of the first elements of the spermatozoa of a mammal, contains also drawings of the first elements of the ova of a singing bird.

Equivalent Products of the Testis and Ovarium.

From the plate now referred to we may, I think, draw something like the following conclusions, viz., that

In the Testis.	*In the Ovarium.*
A large cell, $\frac{1}{100}'''-\frac{1}{80}'''$, A, fills with nuclei, B, the inner part of each of which forms a spermatozoon, C, while the outer part becomes the "developing cell" of authors, D. The head of the spermatozoon is a nucleus, E, which subsequently exhibits a nucleolus, F, the fecundating element.	A large cell about the same size, A, fills with nuclei, B, the inner part of each of which is a young ovum, C, while the outer part becomes my ovisac,[2] D. The germ spot in the ovum is a nucleus, E, which subsequently exhibits a nucleolus, F, the part fecundated.

I have just mentioned an ample confirmation by R. Wagner and R. Leuckart, of my views on the physiological importance of the nucleus of the cell. The following remark is by the same distinguished authors: "It might not be without interest to reflect upon the important part which the nucleus plays in the formation of the spermatozoa, since it is an element which is usually only important for the formation of cells, and does not participate in their subsequent metamorphoses."—*Lc.* p. 501.

This remark shows the existence still of views regarding the nucleus, quite unworthy of it. But when the authors add, concerning the said remark, "This is at least the rule; a rule, however, by no means without exception" (*lc.* p. 501), they give evidence of having seen the nucleus doing something more than simply entering into the formation of the cell. They further say: "We already know that in many cases the nucleus is important for the development of certain parts" (*lc.* p. 501), which they mention; and they add, "We know that the nucleus . . . . even converts itself . . . into peculiar fibrous formations—into the so-called nuclear fibres (Kernfasern).[3] Still more remarkable is the metamorphosis of the nucleus in the development of the so-called prickle or nettle organs—those interesting microscopical formations, which are so frequently imbedded in the skin of the lower animals (*e.g.* polyps and medusæ), and which present so great a similarity to certain forms of the seminal fibres, that they were even taken for such by one of us on their first discovery. Kölliker's observations, as well as our own more recent ones, instituted upon hydra, convince us beyond doubt *that it is the nuclei* of cells which gradually metamorphose themselves into the capsules of the prickles, and which ultimately become free through the dissolution of the cell membrane surrounding them. . . . On examining the external coverings of hydra, we shall readily be enabled to convince ourselves of the formation of these organs. The most different stages of development may here be seen; viz., developed prickles, either free or still enclosed by a cell membrane, from which the organ itself, and *especially the fibre enclosed in its interior, recedes more and more, until it finally appears as a mere simple nucleus*."—*Lc.* p. 502.

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[1] Phil. Trans., 1841, Plate XXV., fig. 161½. It may be deserving of remark that these observations of mine were made on the elements of spermatozoa found—not in the male, but in the female—in the vagina of a rabbit killed two hours *post coïtum*.

[2] The ovisac acquires a vascular covering, and there is thus formed a "Graafian follicle."—Phil. Trans., 1838.

[3] Vid. Henle (Allgemein. Anat. s. 193) and Zwicky (Metamorphose der Thrombus).

Compare this with drawings of mine published many years before, viz., in 1842, of fibre given off by the nucleus of the cell.[1]

The fact that secretion takes place in cells, was first announced by Professor John Goodsir.[2] He originally supposed secretion to be effected by the cell-membrane; but when subsequently becoming acquainted with what he is pleased to term "that most important observation of Martin Barry, on the function of the nucleus in cellular development," he attributed secretion to the nucleus.[3]

Professor Quekett has truly represented each cell of a plant "as having an independent or individual existence," and shown that "in one situation it may secrete *colouring matter*; in another *starch, gum, sugar, oil*, etc.; and in another, the *material for the reproduction of its species*" (*lc.* p. 113). I would, however, add, that it is the *nucleus* of the cell that does all this; one main purpose for which the cell is formed being, I apprehend, the performance within it of the functions of the nucleus, and the resolution of the nucleus into its many forms.

It is now well known that the elements of some tissues consist of caudate, fusiform, stellate, many-rayed, etc., "cells," as they are called. And it is usually said that their processes split up, so that "what was once the cell-wall becomes a mass of fibres." Quekett, *lc.*, p. 129.

I long since investigated with some care the mode of origin of such caudate and fusiform bodies in one tissue—the *connective*, more general in its distribution than any other. And having reason from what I saw of the mode of origin of similar bodies in other tissues to believe it to be essentially the same in all, I will briefly recapitulate the process. In the Phil. Trans. for 1841, Plate XXII., figs. 108 to 116, from the tadpole, rabbit, and fœtal ox, there will be found sketched from the connective tissue about fifty bodies exhibiting a transition from the simple discoid nucleus to the caudate, fusiform, etc., bodies in question by the following changes:—1. The nucleus has no nucleolus. 2. It presents a single nucleolus. 3. There are two or more nucleoli, and in some instances many. 4. Nucleoli initiating division of the nucleus into as many parts as there are nucleoli, the nucleus becomes a compound body consisting of smaller nuclei, which however differ in their size. 5. Processes are sent out, giving the compound bodies in question a caudate, fusiform, and other shapes. 6. These processes near their place of origin contain *a row of the smallest nuclei.* 7. Of the processes farther from their place of origin, nothing more can be said than that they defy the microscope, and in my drawings are represented in outline only.[1]

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[1] Phil. Trans., 1842. Plate V., figs. 9 to 12; Plate X., figs. 133 to 136, etc.

[2] Trans. R.S.E., 1842.

[3] "Anatomical and Pathological Observations," by John Goodsir, F.R.S.E., and H. D. S. Goodsir, M.W.S. 1843.

I have been thus particular, in order to afford observers the opportunity of judging whether the substance splitting up into a mass of fibres is not something more than a "cell-wall." What that substance really is, I have just said the microscope fails to show. But if nuclei, however small, can be discerned in the processes at their commencement, it does not seem too much to presume the presence originally of still minuter nuclei in their continuations. Now, if the processes are originally filled with nuclei, however small, there can be little doubt that nucleoli perform the same part in them as nucleoli perform in their progenitors, the larger nuclei—*i.e.* that nucleoli effect their cleavage. This cleavage continuing, would soon carry the nuclei beyond the limits of the visible, and the containing processes into a so-called homogeneous state. The nucleus, as I have shown, gives off fibre. Where a mass or fasciculus of fibres is to arise, I have found the nuclei to arrange themselves in parallel lines, as in the young crystalline lens.[2] So that if from analogy we may extend what is easily seen in the lens to fibres of the connective and other tissues, it would seem to be from *cleavage of a line of nuclei* that we get a mass of fibres, and not from the splitting up of a cell-wall.

I have stated the processes near their place of origin to *contain* a row of nuclei. This implies my belief in the presence of a substance holding them together, or rather in which they are contained, proceeding from the investment of the nucleus.

Changes of the same kind as those now described as producing the caudate, fusiform, etc., bodies out of simple nuclei, I met with in other tissues. In the crystalline lens I saw, in the first place, large nuclei arranged with great regularity in lines.[1] In a later stage, such nuclei had become composed of smaller ones.[2] In a stage still later the compound nuclei had elongated, so that the smaller nuclei of which they consisted were arranging themselves *in lines* to form the fibres of the lens.[3] One of these lines is comparable to one of the processes we have been considering. And what I have just said of the material split up, of the way in which it would seem that the splitting up occurs, and of the investment, I would apply here, as well as to the formation and reproduction of all other organic fibre where a splitting up occurs, including woody fibre.[4]

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[1] Several German observers, the first of whom I believe was Gerber, have lately seen the true nature of the fusiform, etc., bodies in question, and proposed to call fibres proceeding from them *nucleus*-fibres. Kölliker, however, is not one of these, for he recommends the disuse of the term.

[2] Phil. Trans., 1841, Pl. XXIV., fig. 150.

Certain nerve-"cells" furnish examples of bodies having a fusiform, etc., shape, which I believe to be nuclei and not cells. I am desirous of bringing these under the notice of observers on account of the physiological importance of the nucleus, and especially that of the nucleolus. For if such nerve-cells are still nuclei, it is part of a nucleus and not merely cell-contents and a cell-wall that enter into the formation of *their processes*. And if such nerve-cells are still nuclei, the pellucid body they contain is a vesicular *nucleolus*, no doubt continuing to perform its all-important functions, though in what way we have yet to learn, or perhaps may never know.

In the tadpole, I investigated minutely the mode of origin of the epithelium and pigmentum nigrum in the tail, and that of the pigmentum nigrum in the eye; and refer to drawings I gave[5] as showing stages earlier than those of pigment figured by Quekett. My drawings of pigment, I think, explain the mode of origin of what he has well described in stages more advanced.

The "black granules" of Quekett, in pigment, I found to have their origin *in the smaller nuclei*, which the original nucleus gives off in stages earlier probably than those described by him.[6]

Professor Quekett remarks:—"In some plants, and especially in the *Canna bicolor*, the spiral vessels are remarkable for the occurrence of longitudinal as well as spiral fibres." (*Lc.* p. 98.) And he illustrates this by fig. 86, *c*, *d*, *e*, *f*, *g*, *h*. I have already stated my conviction that the spiral fibre in spiral vessels is given off by the nucleus, and, indeed, that all the forms of vascular tissue are referrible to altered nuclei. Now, in plants such as the *Canna bicolor*, presenting *longitudinal* as well as spiral fibres, it appears to me that analogy points out the following as the mode of origin of the former. The longitudinal fibres occupy in the vessel a position internal to

[1] Phil. Trans., 1841, Pl. XXIV., fig. 145 to 148.

[2] Ditto, fig. 149.

[3] This was in a large tube formed by coalescence of embryonic cells. Phil. Trans., 1841, Pl. XXIV., fig. 150.

[4] See my observations on the mode of reproduction of flax. Phil. Trans., 1842, p. 105, and figs. 75, 76, 113, etc.

[5] Phil. Trans., 1841, Plate XXI.

[6] For such earlier stages see Phil. Trans., 1841, Plate XXI., figs. 93, 90, 91. See also my drawings of the elements of connective tissue, referred to in a preceding note.

the spiral fibres. After the nucleus has given off the spiral fibre, its inner part becomes fusiform and splits up into the longitudinal ones, which thus come to lie upon the inner surface of the first.

"A matrix, lastly," says Kölliker, "is found in cartilages and bones as a solid, homogeneous, granular, or even fibrous substance connecting the cellular elements, and for the most part arising from the blood, independently of them." And in an immediately following remark, it is added:—"The occurrence of a solid blastema, growing independently, in the villi of the chorion and of a solid matrix deposited directly out of the blood, demonstrates that all parts of the body are not, as Schwann was disposed to believe, without exception developed from cells or in dependence upon cells. . . . It is certain that in pathological formations such masses very frequently occur, fibrinous exudations becoming changed in great measure, without previous organisation, *i.e.* cell formation, into permanent tissues."—*l.c.* p. 11.

This harmonizes with what I have been endeavouring to demonstrate, viz., that the formation of fibre by no means implies the previous formation of what is usually called a "cell." Fibre may form within a cell, for instance in cartilage; it may form, according to me, independently of cells from the fusiform bodies we have been considering; and as Kölliker states, it may form, independently of cells, as "a solid matrix deposited directly out of the blood." But I must repeat—when fibre forms within a cell, I find it to be the *nucleus* that forms it; when it forms independently of cells from the fusiform bodies in question, it is still, as I believe, a *nucleus* that forms it; and though a solid matrix of fibre be deposited, as Kölliker remarks, "directly out of the blood," independently therefore of cells, and without proceeding from large fusiform nuclei—yet it cannot be shown that what the blood deposits does not contain, derived from the corpuscles of the blood, immeasurably minute nuclei to form the fibres.

Another of the principal facts recorded by me in the Phil. Trans. for 1842 was, *that the muscular fibril, when fully developed and acting, consists of two spirals.*

On the subject of muscle, Professor Kölliker writes as follows:—"As regards the notion adopted by Bowman, Dobie, and others, that the fibrils are constituted of still more minute particles (sarcous elements), it may perhaps be stated, as the study of development shows, that the fibrils do, in fact, at first appear to consist of separate particles. But the question is, whether in the adult such elementary particles continue to be evident, and this, at present, I am inclined to deny." (*l.c.* p. 239.) "To Bowman's opinion, moreover, is opposed the fact, that his assumed 'elementary particles,' except in macerated muscles, where such a thing readily occurs, can only with difficulty be obtained in an isolated form, whilst, according to his view, such a disintegration, in cases where these particles do not cohere firmly, either in a longitudinal or transverse direction, would necessarily take place with equal facility in either."—*Lc.* pp. 236–237.

In fig. 36 *b*, Kölliker gives a drawing of a muscular fibril, and remarks: "The fibrils are generally enlarged at regular intervals, so that they appear to consist of a series of many portions, and give a transversely striated aspect to the muscular fibres." (*Lc.* p. 91). The drawing is reproduced by Kölliker in fig. 89 *b*, and he subsequently says that it is still doubtful "whence the appearance of transverse striation in the fibrils themselves arises; whether from their being *spirally twisted* (Arnold), from *zigzag curvatures* (Will), or from varicosities. All that I have seen" (he adds), "leads me to adopt the latter view, which is also that most generally entertained. . . . . . . Quite lately, Dr Barry has propounded the view that each muscular fibril is constituted of two spirally convoluted filaments running in the same direction. I have seen nothing of the kind, and do not hesitate to describe the whole of Dr Barry's exposition as nothing but a myth, and his figures as fantastic images."—*Lc.* pp. 238-239.

As more than a score of drawings in the Phil. Trans. for 1842 show that I had then seen the transverse striæ in muscular fasciculi to arise from *the structure of their fibrils*, I have now, from the foregoing paragraph, the satisfaction to find, as his fig. 89 *b* also shows, that Kölliker and I on *this* point are agreed.

Regarding the structure producing the transverse striation in the fibrils, however, it is evident that the worthy professor and I differ widely, as he has most unequivocally shown in the last line of the paragraph last quoted. Nor am I at all surprised at this. My experience during the last twelve years enables me to say that Kölliker is but one of very many who have regarded my views on this vexed question just as he does. Perhaps the day may come when he and they will wish they could expunge what they have written; for facts are stubborn things. I do not expect, however, that this will happen until observers have directed their research patiently and in earnest to muscle, where, as in the tail of the *youngest* tadpole, the fibrils are very large. In older muscle, minuteness alone would constitute a difficulty. But here, what mainly complicates the structure is *continued change*—old fibrils, as contractors, worn out, and young ones succeeding—old fibrils passing into that *apparently* homogeneous, structureless state which I have described as constantly seen to be assumed by other fibre; and young fibrils in every phase of quadrilateral, oblong quadrilateral, and rhomboidal particles—and these undergoing not only division and subdivision, but dislocation also, until at length they reach the twisted, and then the twin spiral form.[1] No wonder if old and young fibrils, mixed up in all these phases, present to those who have begun the investigation at the wrong end a structure more ambiguous than any other.

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[1] See a paper of mine entitled, "*A Main Cause of Discordant Views on the Structure of the Muscular Fibril*," in the Phil. Mag. for Nov. 1853.

The drawing, however, of a fibril given by Professor Kölliker, figs. 36 *b* and 89 *b*, was taken, it appears, from one of the perennibranchiate amphibia, where, as he says, the fibrils are large, and perhaps "above all others adapted for this investigation." (*l.c.* p. 239). I have already said that that figure shows transverse striæ in muscle to arise from the fibrils. But that figure shows something more than this, and gives proof how well adapted the perennibranchiate amphibia are for the investigation; for, in a German edition of Kölliker's "Histology," that very figure was pointed out to me by Purkinje himself, with a remark to this effect: "Da sind Ihre zwei Spiralen" ("there are your two spirals"). He saw, and I saw with him, that a structure which, nearly every day for months, I had the opportunity of preparing anew, and demonstrating to Purkinje, had been seen and delineated, but not understood by Kölliker.

This observer errs in supposing my views on the spiral structure of muscle to have been propounded "quite lately" for the first time; a proof that, to continental physiologists, what is published in the Philosophical Transactions often remains unknown. It appears that he became acquainted with my views through a paper in Müller's Archiv. Heft vj. for 1850, without observing that what that paper contained was only the account of a *renewal* of inquiries on a subject, regarding which my views had been recorded eight years before, viz., in the Phil. Trans. for 1842; and he omits to say that the said paper was translated from the English, and communicated to the Archiv. by—a PURKINJE. I appeal to all who have the honour of knowing, in connection with the microscope, the rigorously investigating, most cautious, and circumspect Purkinje, whether there lives the man less likely to be misled by "a myth" or by "fantastic images" than he.

The fibre forming vegetable membrane, was in 1852 delineated by Agardh as having a twin *spiral form.*[1] This in 1842 I had shown to be the very form of the fully developed and acting muscular fibril, as well as the very form of the axile fibre in nerve. And two hundred drawings of mine, then published,[2] show that I had been unable to see any difference between the markings *originally* presented by organic fibre in general, and those of muscular fibrils of about the same size.

THE SPIRAL FORM.

One of my announcements that met with absolute derision was, the presence everywhere of spirals. This fact, I confess, seemed marvellous enough to me, and it was not until, as it were, compelled by accumulated observations, that I ventured to make it known. Now, the observations of Agardh may be referred to as a confirmation.

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[1] "*De cellula vegetabili fibrillis tenuissimis contexta.*" Lundæ, 1852.

[2] In the 157 *figures* then engraved—Phil. Trans., 1842. The arrangement of many of those figures is not such as to bring together all those of the same tissue, but simply to show identity in the size and structure of parts composing tissues *of all sorts.* Thus, for instance, in Plate VIII., figures of spirals of the cornea, of muscle, and of nerve, are mixed up with those of vegetable spirals.

Yet, is the universality of the spiral form so very marvellous after all? I think not, if we consider the spiral in its most incipient state. Cells, or rather the elements of cells, I suppose are now acknowledged to be universal. By the elements of cells, I of course mean nucleolated nuclei. Why *this* uniformity, we do not inquire. Yet, if elements with such a form are universal, it surely is not so very marvellous that the *first changes* in their form should be the same. Now let us see what one of these first changes is. I refer to many scores of drawings of mine from nature in the Phil. Trans. for 1841, in proof of the frequency with which the nucleus becomes a *biconcave body, or a ring.* Divide a ring at any part, and on lateral separation of the ends, you have an incipient spiral. It was such ring-like nuclei and the crescentic halves thereof, that I saw passing into spirals. There the spirals were of considerable size. But if such be the mode of origin of spirals that can be discerned, does not analogy forbid the supposition that even the minutest spirals have an essentially different mode of origin? So that it being ordered that every structure shall have its origin in bodies of the same form—the elements of cells, it does not seem more marvellous that there should be great uniformity in the primary arrangement of the same, especially when it leads to the formation of one thing—fibre. And be it remembered, this arrangement in animal fibre is in nearly every instance as transitory as those elements themselves.[1]

CLEAVAGE IN THE MAMMIFEROUS OVUM.

The first two cells of which, after the disappearance of the germ vesicle, the new being is composed, correspond to what observers of ova of some of the lower animals have termed the first two "cleavage masses." Judging from drawings given by others, I think there is little doubt that the ova of some animals present true cleavage. The best evidence of this that I have met with, is in Nelson's figures of ova of *Ascaris Mystax.*[2] But, because not undergoing literal *cleavage,* does the mammiferous ovum essentially differ from other ova? Certainly not. The difference seems to lie in this: that where literal cleavage takes place, there is an external solid substance of secondary importance left—a true yelk—to cleave; whereas, in the mammiferous ovum there is none.

I adhere to an opinion expressed in the Phil. Trans. for 1839,[3] that in the mammiferous ovum the "yelk," as it is called, corresponds to no more than the substance termed the *discus vitellinus* in the ovarian ovum of the bird. And perhaps in the ovum of the bird a process in that *discus vitellinus* may be hereafter seen, not differing essentially from the process I described as seen in the ovum of mammalia. For the operation of such a process, and for rotation on its axis of such a ball, it may be that in the true yelk of birds there exists a large central cavity of fluid, between which and the surface there is a canal provided for passage and return. This implies my belief that the said *discus vitellinus* in the bird's ovum undergoes as much literal cleavage as the so-called "yelk" in the ovum of the mammal, and no more; that is to say, none at all.

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[1] In the muscular fibril the twin spiral form is permanent, until this fibril as a contractor is worn out.

[2] Phil. Trans., 1852.

[3] P. 370.

DIFFERENCE BETWEEN NUCLEI AND CELLS IN REGARD TO CLEAVAGE.

"*Cell*-division" is an unfortunate expression. A *nucleus* undergoes true cleavage, never seen, I believe, in the membrane of a *cell*. I think it possible thus in some instances to distinguish a nucleus from a cell, which by other means is often no very easy matter.

This cleavage of a nucleus sometimes takes place within a cell, and sometimes a nucleus is cleft without having formed what can be called a cell.

1. *Cleavage of the nucleus within a cell.*—The nucleus here is cleft either when quite small, or not until it has enlarged; and sometimes not until it fills the cavity of the cell. In the latter state the nucleus is the folding in primordial utricle of Von Mohl.

2. *Cleavage of the nucleus which has not formed a cell.*—The circulating red blood-discs of mammalia are nuclei. These are constantly seen becoming segmented. The segments are set free, and separate from one another. This is an example of cleavage into about half a dozen parts.

Size does not enable us to distinguish nuclei from cells. Nuclei may become very large, and yet continue nuclei. Examples of this are afforded, I think, by the protozoa; my belief being that these *are enlarged nuclei and not cells*, the minute pellucid body they contain being a nucleolus which takes part in, and no doubt (like other nucleoli) initiates that cleavage. Cilia-bearing cylinders are, I think, cleft nuclei,[1] like some of the vorticellæ which they resemble also in the position of the cilia. And each epithelium cylinder remains a nucleus.

The very ovum itself, that is to say, the yelk-ball, I conceive to be a nucleus of enormous size. If so, my ovisac is its cell. (Sometimes, as is known, several ova are found in one ovisac;[2] so also are several nuclei sometimes found in an ordinary cell). The germ vesicle, its filling with cells, and the process operating in the early development of the ovum, furnish remarkable examples of the formation and reproduction of *cells within a nucleus*. Farther, the true cleavage of the yelk-ball in certain animals, admits of comparison with the cleavage of other nuclei.

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[1] Phil. Trans., 1841, Pl. XXI., figs. 96, to 100.

[2] This I have elsewhere shown to result from division of the nucleolus before the formation of the cell, and when there exists merely a nucleolated nucleus. Edinburgh New Phil. Journal, Jan. 1854.

"*Cell*-division," I repeat, is an unfortunate expression, it being only *nuclei* that are cleft. Yet there is but one kind of reproduction, viz., the fissiparous. For, though the membrane of the cell undergoes no actual cleavage, a mother cell does not reproduce without cleavage of its nucleus.[1]

Kölliker seems altogether to deny true cleavage in the ovum. Instead of cleavage, he supposes his primary nucleus, the pellucid centre of the yelk-ball, to "operate as a centre of attraction upon the yelk" and unite into "a globular mass, the first cleavage mass;" and he thinks that then the nucleus becomes resolved into two halves, these acting "as new centres upon the yelk," and thus breaking up the first cleavage mass into two. Each of these has its nucleus, which also becomes resolved into two halves. These in their turn acting as centres of attraction, double the number of yelk masses, thus increased to four, and so on. The editors of Kölliker "altogether demur to the notion that the 'nuclei' of the dividing yelk exercise any *attraction* upon the yelk substance," and for "demonstrative evidence that no such attraction exists," they refer to the observations of Reichert,[2] of Remak,[3] and of Nelson.[4]

It appears to me far from improbable that in the ova which Kölliker has particularly investigated, there may take place no true cleavage. But with his editors I altogether demur to the notion of "attraction" in its stead. I venture to believe that what Kölliker had before him and referred to attraction, was no other than the very process I described in the mammiferous ovum. Thus the "small globules" of Kölliker are the product of my transitory cells. And his "new nucleus, the primary nucleus of the embryo," is what I maintain to result in mammalia from the fecundated centre of the germ spot.

As to membranes being formed *around* the "cleavage masses," by which the latter become actual cells, as supposed by Kölliker, this is certainly a mistake. The very title of a paper by Reichert, mentioned by the editors when demurring to Kölliker's notion of attraction, shows that he denies the formation of a cell *around* the "cleavage masses."[5] And as to "a new nucleus" arising "around a new *nucleolus*," nothing of the kind takes place in the mammiferous ovum, nor anywhere else. Farther, for a refutation of Kölliker's idea, "that the investing globules developed by the successive multiplication of nuclei, become cells only in the latest generations," the process, according to him, being in the earlier stages "nothing else than an introduction to cell-development," I refer to a host of drawings of my own, of what were seen to be *true cells in the earliest generations.* And with regard to Kölliker's declaration, that "the germinal vesiele, in most cases, has nothing to do with it" (the supposed introduction to cell-development), it is sufficient to refer to what Johannes Müller saw in the molluscs developed within *synapta digitata*, where, he says, the germ vesicle *does not disappear*, *but yields the pellucid bodies in the interior of the cleavage masses.*[1]

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[1] See a Paper of mine, "On fissiparous generation." Edinburgh New Phil. Journal, Oct. 1843.

[2] Müller's Archiv., 1846. [3] Müller's Archiv., 1851. [4] Phil. Trans., 1852.

[5] The title of Reichert's paper is: "Der Furchungs-Prozess und die *sogenannte*," [so called] "Zellenbildung um Inhalts-portionen."

CELLS OF THE OVUM COMPARED WITH OTHER CELLS.

I am indebted mainly to my researches on the cells of the ovum for what little I know of other cells. From their large size, the eye could penetrate into the former; but the latter, if seen first, would have yielded only isolated observations, leading to no general results, and by their usual minuteness, the microscope, in most instances, would have been defied.

In the mammiferous ovum, the foundations of young cells arise in no other way than by division of the nucleus of a mother-cell; and of myriads of young cells that form, all disappear excepting two.[2] More particularly described, yet still in a very general way, and without mentioning many details, the process is as follows:—The nucleolus becomes nodular, gives off its nodules, and thus segments the nucleus; the segments are given off in a zone. The nucleolus supplies their place by a fresh segmentation, and out of this there arises another zone internal to the first, and so on. The segments are young nuclei. These form cells. So that there arise concentric zones of cells.[3] At length, the inmost part divides into halves, and these two halves become two cells, at the expense of all the previously formed cells, which disappear.

No one can see the first two ovum cells ("cleavage masses") when about to give origin to the nuclei of four,[4] without being struck with the resemblance they bear to the red cells circulating in the blood of batrachians,[5] except that they are not flat. In their elliptical form, in their eontents, and in their pellucid nuclei, they are very much the same.

Farther, no one can have made himself acquainted with the process just mentioned, as reproducing the ovum cells, without recognising more or less of that very process in other cells. And this not only in other *reproducing* cells, but in newly formed cells arising for whatever purpose. On this subject, see my papers in the Phil. Trans. for 1841. And in these, see especially the drawings from nature illustrative of the mode of origin of the epithelium tables and cylinders, and the pigmentum nigrum.[1] On comparing these with my drawings of the ovum cells just referred to,[2] the reader will find abundant evidence that the process is the same in both.

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[1] Müller's Archiv., 1852. S. 19. For other confirmations see the Edin. New Phil. Journal, Jan. 1854. See also in the Phil. Trans. for 1840, my fact, maintained in opposition to all previous observers, that the germ vesicle does not disappear at the period supposed, but persists for a purpose which I there described.

[2] Phil. Trans., 1840.

[3] The interior of these secondary cells presents a process of the same kind.

[4] Phil. Trans., 1839. Plate VI., fig. 105½.

[5] Before these blood-corpuscles have been acted on by a chemical reagent.

ASSIMILATION.

Why, in the development of the ovum, the formation of many cells which for every generation come into existence and speedily disappear by entering into the formation of the more aristocratic two? Why all this cell-life? I apprehend, with Carpenter, that the process is one of *assimilation*. Where cells are merely reproducing cells, their membrane is the wall of a living laboratory raised for the performance of its own functions by the more essential part.

THE NUCLEOLUS.

The ovum cells *absorb*, *assimilate*, and *reproduce*. So do other cells. In these functions the prime mover is the *nucleolus*.

1. The nucleolus *absorbs*. The most remarkable example of this, is the suctorial power manifested by the protrusion of the parietal uncovered nucleolus of the germ spot into the aperture of the vitellary membrane, to take up the equivalent, corresponding, or counterpart, from the spermatozoon.[3] But where is there to be found a cell in which the nucleus is not at first *parietal* in its cell? And where does there exist a nucleolated nucleus in which the nucleolus is not at first *parietal* in its nucleus? Another proof of absorption by the nucleolus is furnished by the originally flat form of the nucleus. The mammiferous blood-discs or nuclei are even biconcave; a provision perhaps for absorption by the nucleolus on both sides. And I have only to refer to embryonic blood, for instance to that of the fœtal ox about an inch in length, for cells whose membranes at the part where the nucleolus lies present, like the germ vesicle, an *orifice*.[4] In mere nuclei, too, it is often seen that at the very part where the nucleolus lies the nucleal investment ceases—*the nucleolus lying uncovered and open to the exterior*.[5]

2. The nucleolus *assimilates*. This is shown in the ovum cells by the germ-spot process. There it is the nucleolus that initiates and completes every change. As I have already said, it becomes nodular, gives off its nodules, thus segments the more or less dis-

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[1] Phil. Trans., 1841. Plate XXI., figs. 85 to 100.

[2] Phil. Trans., 1840.

[3] "It is the mysterious centre [nucleolus] of a nucleus which is the point of fecundation, and the place of origin of two cells constituting the foundation of the new being,"—Phil. Trans., 1840. P. 557.

[4] Phil. Trans., 1841. Plate XVII., fig. 28, α, α.

[5] Phil. Trans., 1841. Plate XVII., fig. 29 δ. Plate XX., fig. 63 α, β, γ.

tinctly vesicular nucleus, and thus initiates the formation of concentric zones of cells. By absorption of the fecundating element, the nucleolus becomes a third substance. This repeats the germ-spot process, though no longer in a parietal part, but *in a centre.* It becomes nodular, gives off its nodules, and thus initiates the formation of concentric zones of cells. Such continued *giving off* of its own substance—ever passing as it is from nucleolus into nucleus—implies an equal *taking up.* And what can it take up but that of which it is the centre? This it does take up, and is at length in a condition to reproduce itself by division into halves. What can the purpose of all this be but assimilation? In proof of assimilation by the nucleolus in other cells, I have only to refer to its change from a parietal to a central position in the cell—to the nodular nucleoli—to the segmented nuclei—and to the concentric zones in cells, of which I gave drawings from cells of the lobster, tadpole, cod, chick *in ovo,* rabbit, and fœtal sheep and ox.[1] This continued origin of new substance in the centre, pushing previously formed zone-like substance further out, serves, I think, to show that a nucleolus is ever passing into the state of nucleus. And farther, it seems to show *how* it is that, as was observed by Remak, the primordial utricle of Von Mohl is a nucleus enlarged.

3. The nucleolus *reproduces.* The cells of the ovum, as we have seen, give origin to daughter-cells. But according to my observations, no daughter-cells arise except through division of the nucleus of the mother-cell. And no nucleus divides until its *nucleolus* has divided. Compare this with a host of drawings I gave of blood-cells, of the corpuscles of pus, mucus, and epithelium, as well as with the elements of the connective tissue, the crystalline lens, and nerve.[2]

The phytozoon I have in a former page stated to be the nucleus of a cell. So that the mode of origin of a spermatozoon and of a phytozoon is the same as that of fibre, when the latter arises in a cell. From what I saw of the mode of origin of cilia also, it appears to be essentially the same.[3] I here refer to stationary cilia. For what are spermatozoa and phytozoa but locomotive cilia carrying about their bulbs?

We may go much farther. Compare, for instance, the young in the ovum of certain intestinal worms with my drawings of the coil of fibre given off by the nucleus in its cell.

Even in mammalia I found the so-called "primitive trace" or rudimental embryo to be no other than the altered nucleus of a cell. The history of this cell is as follows:—Cleavage, so-called, which was known to take place in the ovum of some of the lower animals,

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[1] Phil. Trans., 1840 and 1841, and Edinb. New Phil. Journal, 1847. Plate I., fig. 39.

[2] Phil. Trans., 1841. Plates XX. to XXV.

[3] Phil. Mag., Sept. 1852.

I found to take place in that of the highest class mammalia. I have already stated this cleavage to be no other than a process of cell-formation. With every division the cells are doubled in number, and each brood is smaller than the last. At length they become innumerable and very small, and are densely grouped into a mulberry-like mass which occupies the centre of a cavity containing colourless transparent fluid. Of this mulberry-like group of cells I gave many drawings.[1] It expands into the so-called germinal membrane, thus coming to line with cells the zona or vitellary membrane. This expansion reveals the existence of a cell in the centre of the group far larger than the rest, and in some instances having a diameter many times that of the cells around it. This large cell, passing to the surface, comes to lie immediately under the vitellary membrane. The "primitive trace" or rudimental embryo is no other than the altered nucleus of this large cell. Of this cell I gave many drawings, as well as of its nucleus.[2] These show the gradual alterations by which a round nucleus becomes the club-like "primitive trace," or rudimental embryo. They do not essentially differ from the alterations I found to take place in other nuclei, consisting in the formation of zones continually pushed out from a centre. In no instance did I obtain more abundant evidence that changes in and enlargement of the nucleus are initiated by the nucleolus, than was furnished by the very nucleus in question. My drawings show that not only when the nucleus is round, but after it has assumed the club-like form and become the "primitive trace," *there is a continued origin of new substance in the centre* (*nucleolus*), "parts previously situated there," as I said, "having been *pushed farther out*."[3] Each zone is made up of segments, each segment a young nucleus. Thus the alterations here, as in every other nucleus, consist in continual divisions.

This nucleus, then, being the embryo itself, I conceive that in physiological importance there is no nucleus that can be called its rival. In it we have the main result of that wondrous process of transitory cell-life in which really consists cleavage of the ovum. That process in assimilating spermatozoon and germ-spot, produces the nucleus in question—a third substance, which, thus composed of elements derived from both parents, begins forthwith to be developed into offspring inheriting properties from both. How this inheritance is brought about, we probably shall never know. But there is one thing perhaps not undeserving of notice, which is this: that, as the simplest forms of independent life are reproduced and perpetuate their race by fission, so by fission are reproduced the nuclei composing the most complicated organism; the first of these being, as we have just seen, a union of elements derived from both parents.[1]

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[1] Phil. Trans., 1839. Plate VI. Ib. 1840. Plates XXVI., XXVII.

[2] Phil. Trans., 1839. Plate VI. figs. 113, 114, 115, 116 bb.; Plate VIII., fig. 148. Ib. 1840. Plate XXVII., figs. 223 to 228, 232 to 234.

[3] Phil. Trans., 1839. Sects. 212, 266, Plate VIII., fig. 148; Plate VII., figs. 118, 121 *a*, 121 *b*.

Expressed in the fewest words, my leading observations brought together in this communication are as follows:—1. The nucleus neither remains inert, nor is absorbed as useless. 2. It is a nucleus that contains the fecundating element. 3. It is a nucleus that receives the fecundating element. 4. Certain phenomena are presented by the nucleus. 5. These occur in a certain order. 6. They occur apparently in all nuclei. 7. They seem to imply absorption, assimilation, and secretion by the nucleus. 8. They are attended by continual cleavage. 9. They thus lead to reproduction of bodies having the same form—that of a nucleus. 10. And they thus lead to the production of parts which are so numerous and so different that all other parts may be supposed to have their origin in nuclei. 11. The prime mover in all these changes is that which has been called the *nucleolus*, the part which in mammalia receives the element of fecundation.

While in thus maintaining the importance of the nucleus to be beyond that of cell-membrane and "contents," I differ from Schwann as to what is the essential part of the cell, it will be obvious that, so far from tending to lessen the physiological importance of his great discovery, my facts add proof thereof. And farther, it will be seen that these facts of mine, locating as they do the prime mover in the nucleus, are about as directly opposed as possible to the views of a recent author of great talent, who conceives the value of the cell-theory of Schwann to be purely anatomical.

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[1] It is remarkable that Bischoff, when confirming my observations that cleavage, so-called, takes place in the mammiferous ovum, and leads to the formation of a mulberry-like group of cells, should have failed to see in the centre of the group a cell larger than the rest—its nucleus the important body now referred to—the very embryo itself. He saw, as it were, the nut-*shell*, but did not see the kernel. Perhaps the day may come when Bischoff will see the latter too. For he has lately contributed to the establishment of a fact of cardinal importance, which for a dozen years he had joined others in denying—the penetration of the spermatozoon into the interior of the ovum. This fact I recorded in the Phil. Trans. of the Royal Society of London for 1840; established it by further observations published in the Phil. Trans. for 1843; and in the Edinburgh New Philosophical Journal, for October 1843, gave a drawing of an ovum of the rabbit, containing in its interior nine spermatozoa, with the statement that I once counted more than twenty in an ovum of this animal. These facts of mine were neglected or denied until 1852, when in several of the lower animals they were confirmed by Nelson and by Newport in this country, and especially by Keber in Germany, the paper of which indefatigable observer, it seems mainly to have been that induced other physiologists on the continent to enter upon renewed inquiry. At length, in 1854, spermatozoa were seen within the zona of the mammiferous ovum. This was done by R. Wagner, Henle, Baum, and Meissner, and even by Bischoff himself. And the latter, in a paper dated 25th March 1854, now candidly acknowledges, that in denying the penetration of the spermatozoon into the interior of the ovum, he had done injustice to Barry, the mistake having been his own. There are other things that Bischoff has yet to learn, concerning which he wrote that "Barry had seen too much." I have just mentioned one of these—the existence in the mulberry-like group of a cell larger than the rest with its all-important nucleus. Of other facts of mine, still denied by Bischoff, may be mentioned the following, viz., the escape of the ovisac from the ovary; the existence of bodies truly deserving the name of retinacula from the office they perform in regulating the position of the ovarian ovum of mammalia; the persistence of the germ vesicle beyond the period when it had by all been supposed to disappear; the part taken by the germ-spot in the production of the new being; and the fact that cleavage in the mammiferous ovum is effected by means of cells.

POSTSCRIPT.

Kölliker mentions Remak as the first who observed reproduction of the blood-corpuscles by division—an observation first published by Remak in 1841. "Yet," says Kölliker, "he subsequently retracted his opinion; and only now, since I have confirmed it, and declared it to be true . . . . has he again advocated it."—*l.c.* p. 29.

When Kölliker wrote this, he was evidently not aware of observations recorded in the "Medical Gazette," Nov. 13, 1839; for these show that the first intimation of reproduction of the blood-corpuscles by division was given by Professor Owen. And it may be added, that the next record of such observations was by myself, in the Phil. Trans. for 1840. See also a paper of mine in the Phil. Trans. for 1841.

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